EXPERIMENTAL METHOD IN CLINICAL PSYCHOLOGICAL PRACTICE*

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PERHAPS in no other applied discipline are there to be found so many differences of opinion as exist in clinical psychology today. Clinical psychologists in fact even disagree about the fundamental approach to their own subject. Let us examine briefly what appear to be the major approaches.

As Eysenck (2) has pointed out, many clinical psychologists who work in mental hospitals appear to accept the role of a technician. They accept both the conceptual system and the practical methods of the psychiatrists they work for. Essentially they seek to answer questions of diagnosis and aetiology by means of tests which have grown out of psychiatric theory and which have usually been validated against psychiatric opinion. The implications of the test results are assessed by the doctor in the light of his own theoretical system, and the treatment carried out is not based on any psychological theories or experiments. The clinical psychologist, particularly in America, may assist in this treatment also, giving psychotherapy under medical direction, so that his role becomes completely subordinate. The psychologist in fact is an assistant psychiatrist with a little special knowledge of certain tests, which he gives as directed. This situation would be justifiable, if not very satisfying, if psychiatric theories were well established, and psychiatric treatments of proven efficacy. Unfortunately this is not the case.

The alternative role for the clinical psychologist is that of an independent scientist, who makes his own unique contribution. This relationship is more like that between the physician and the biochemist. The biochemist is an independent scientist who formulates and investigates theories in biochemistry often fundamental to psychiatry. He applies his scientific knowledge and method to answer specific questions put to him by the physician. He is not told what tests and procedures to adopt, nor do his methods and theories assume the *a priori* validity of those of the psychiatrist.

This approach sounds more acceptable to many psychologists as they argue that the basic training of the psychologist is in science and psychology, not in clinical medicine or therapy. There is one basic difficulty, however. In our analogy, the biochemist while pursuing fundamental research of his own, can also *apply* his science to answer routinely specific questions put to him by the doctor. His immediate practical value to the hospital is that of an *applied* scientist. If the clinical psychologist is to be an applied scientist, he also must have a science to apply. Some objective psychologists appear to believe that there exists a science of abnormal psychology which can be applied routinely. They often administer a routine battery of tests to all patients entering the hospital. Whether such batteries have much practical value is doubtful however. In fact it is doubtful whether there *exists* what might be called a science of abnormal psychology at the present time which we can apply routinely. We have

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on the one hand the beginnings of a science of general psychology, and on the other a small body of empirical psychiatric facts and a larger body of untested psychiatric theories. However, we have no general scientifically tested theory about the causes and treatment of psychological abnormalities.

This, then, appears to be the basic dilemma of the clinical psychologist. If he is to accept the role of a scientist he finds himself in the practical situation with no science to apply. It is not so surprising perhaps that some clinical psychologists have slipped into the role of an assistant psychiatrist.

Some clinical psychologists who are often very aware of the present shortcomings of the science of abnormal psychology, and are also dubious about the practical value of batteries of objective tests applied routinely, have tended to reject the scientific role completely. They practice what might be called the *art* of clinical psychology. Some frame of reference such as psychoanalysis is adopted. Using interview procedures, projective tests and sometimes even analysis of handwriting, they attempt to "understand" the patient. Their conclusions and recommendations are based fundamentally on a subjective personal interpretation of the patient's behaviour, making use of general rules of procedure like artists in other fields. The main objection to this approach is that it is not possible to assess the validity of the conclusions arrived at until after they have been acted on, and the results observed. This method of work also depends on the capacity of the individual doing it and there would seem to be no way of establishing this without conducting an elaborate investigation into the validity of each individual psychologist as an oracle. Even if such an individual were shown to be a fine judge of character, we would still not necessarily know how he did it.

Some psychologists who would reject the idea of making clinical psychology an art rather than a science, would maintain that we should stick to fundamental research and not try to do any applied work at present.

Of course we must admit that such basic research is essential. Nevertheless, it does not necessarily follow that we can do no practical work. In fact the dilemma which was suggested is perhaps not a dilemma at all. Science has two aspects. It is a fairly well established body of tested theory and fact, but it is also a *method* of answering questions. If clinical psychology lacks this body of knowledge it can at least apply scientific method to individual problems. We may have no general explanation of a certain psychological abnormality in all people. However, in the course of clinical work an individual might be referred to us with such an abnormality. The abnormality can be investigated scientifically in its own right. We can first seek to define and describe the symptoms exactly, then put forward hypotheses to explain them, make deductions from these and test them experimentally on the individual. We must discard our hypotheses if the predictions are not fulfilled, and erect new ones in their places, testing these in turn. Eventually we may arrive at an explanation of this individual's specific abnormality and, therefore, be able to bring it under experimental control. If we can do this we may be able to cure the abnormality. This does not necessarily account for this abnormality in all people. However, it has not been our purpose to do this. The explanation may of course have implications for more general theories. An experimental investigation of a single case, if successful, will almost inevitably lead to hypotheses with general implications which would merit research in their own right. In this way clinical work can go hand in hand with research, research problems being suggested by experiments on individual cases, and the investigation of individual cases making use of general research findings.

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This general approach to clinical psychology which has been pioneered by M. B. Shapiro (4) is the application of the experimental method to the study of a single case. While it is easy to describe the method in broad outline, it is perhaps not so easy to put it into practice. This is hardly surprising. If the problems of abnormal psychology were *easily* susceptible to experimental enquiry the field would undoubtedly be more advanced than it is. The practising psychologist will find that he has too many patients to conduct elaborate experiments on each one. An experimental investigation of one case takes a very long time. Some of our studies have, in fact, taken as long as a year with some testing being done at least every week. Even at the end we have often found that we have not answered the question we set out to answer. In spite of these difficulties, however, this approach is justifiable for the following reasons. First, if the criticisms of the alternative methods are valid, it is the only thing we *can* do. It is better to accept the fact that we must see fewer patients and spend longer with each, than to resort to simple procedures which have no practical value anyway. Secondly, this method forces one to formulate the problems presented by an individual much more clearly, and this in itself is valuable even though little actual research is done on the individual. Thirdly, even when experimental investigations fail in their ultimate aim, either through lack of time or because of the difficulty of the problem itself, they nevertheless succeed in obtaining some useful facts about the patients which can be used in their rehabilitation or treatment. Furthermore, these facts would not have been discovered if some different approach had been adopted. In the course of disproving a plausible theory about a patient's illness, striking and surprising information often emerges which one would not have been able to predict.

Perhaps the greatest single value of this approach is that we have to formulate clearly the problems presented by the psychiatrist. It is easy to accept glib explanations of abnormal behaviour if they do not have to be tested. If, however, one is forced to formulate these explanations in such a way that they can be *disproved* by an experiment on the individual, one finds that many explanations are not explanations at all because nothing follows from them. Absolutely no predictions can be made from them. Similarly, many apparent problems presented by the psychiatrist turn out to be meaningless or at least very different when one is forced to design experiments to look into them.

The following are two examples of problems which psychiatrists have actually put. On the face of them they seem very similar, and they are typical of a large group of questions clinical psychologists try to answer.

The first request is as follows: "I should like you to give this patient a Rorschach to aid me in making a differential diagnosis between obsessional neurosis and schizophrenia." The second request is: "I should like you to give this patient a Rorschach to help decide whether he is a depressive or an organic case; that is, a pre-senile dementia."

Both are questions of differential diagnosis. Many psychologists would not hesitate. Both patients would be given the Rorschach and a diagnostic label would be suggested for both on the basis of the test. What the psychiatrist would do with this information would be left to him. A more critical psychologist might object to being instructed to give this particular test. He would perhaps feel that there is no evidence that the Rorschach can discriminate between the diagnostic groups in question. He might, therefore, suggest a better validated test giving an answer which quoted a probability in connection with

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either diagnosis. For example, for Case 1 he might say: "In a recent study, only 5 per cent. of schizophrenics obtained so low a score on this test, whereas 50 per cent. of an obsessional group got similar scores. I think, therefore, it is more probable that the patient is an obsessional neurotic than a schizophrenic." It might be felt that the second type of answer is better than the first. However, again it is left to the psychiatrist how to interpret these results.

Neither of these psychologists has performed an experiment, however. Neither has formulated a testable hypothesis about anything. In fact, neither has tried to find out exactly what abnormality he is seeking to explain. If we are to experiment on the patients we must clarify out ideas considerably. Let us take the first question: "Is this an obsessional neurosis or schizophrenia?" We must first discover what the psychiatrist means by the terms "obsessional neurosis" and "schizophrenia", as their meaning tends to differ from psychiatrist to psychiatrist. Secondly, the question may be meaningless anyway. There is no reason why the patient could not be both obsessional *and* schizophrenic, if these terms refer to an extreme position on certain unrelated personality dimensions. Therefore, we must go back to the psychiatrist to find out *why* he is asking the question. After discussion, when the problem is shorn of its psychiatric terminology it might really be: "Why does this patient, an intelligent man, believe that all his work-mates are spreading false stories about him and how can I cure him of this idea and its concomitant emotional disturbance." Now that we have the question we can at least try to answer it. The psychiatrist himself may well have theories about it which we can test. We might discover when these were examined that they were essentially untestable; that is, nothing could be predicted from them. The psychiatrist may believe that psychotherapy might help. The psychologist could then examine the evidence relating to the success of psychotherapy for people with these specific symptoms. The evidence might suggest that this is not a good idea. In the end the psychologist may find that he himself is forced to suggest hypotheses to account for the symptoms, and to test them experimentally. A great number of possible explanations might be formulated and tested by experiment. The investigation would clearly be a long one and success could not be guaranteed. In fact the problem is very much more difficult than it at first seemed. The psychologist is faced with a piece of behaviour which is clearly abnormal and must find some way of explaining it with the hope of bringing it under experimental control, so that it can be modified permanently. A simple routine test will probably be quite valueless in suggesting how this might be done.

The second example, if examined, might be entirely different. The psychologist was asked to help decide between a diagnosis of depression and an organic state, perhaps pre-senile dementia. If the psychologist does *not* give a simple diagnostic test immediately he must re-examine the exact nature of the problem. After careful discussion with the doctor the following picture might emerge: This man of 45 is clearly very depressed and it is assumed that E.C.T. is a cure for the specific symptom of depression. However, on clinical examination, the patient gave signs of some memory impairment and it was thought that this might be due to some brain lesion such as occurs in pre-senile dementia. If this were the case, E.C.T. might make the memory impairment worse and it would not be advisable. The psychologist's course of action is now quite straightforward. At present the *only* evidence for a brain lesion is a clinical impression of impaired memory. The psychologist can find out exactly what questions seemed to elicit memory impairment. He can repeat these under controlled conditions. He can test a control group of patients of this age with

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no brain injury. He might prefer to use a standard test of memory for people of this age if it seemed appropriate. However, the problem is clear. Essentially he is trying to decide whether the patient has a specific disability. He is trying to discover whether the patient's memory is abnormal compared with people of this age. If it is *not*, the psychiatrist has been misled and brain damage need not be postulated. The problem is now merely whether E.C.T. will alleviate the depression. The point about this method of working is that instead of tackling a rather vague diagnostic problem, the suspected abnormality of the patient, i.e. possible memory impairment, is tackled directly by experimental methods.

It is not being suggested that many sensible clinical psychologists would not have gone closely into both these cases and discovered most of the relevant facts. It *is* being suggested that the experimental approach by its very nature forces one to cut through vague terminology which can obscure the issue and, therefore, to discover the essence of the problem before any testing is done at all. In this way problems can be assessed more easily. Some are clearly very difficult to solve but some at least can be answered fairly easily. In a busy hospital, if we are to be of real assistance to the greatest number of cases, we must concentrate mainly on these simple questions. It is useful to *design* an experimental enquiry as a method of assessing which cases the psychologist should tackle even if it turns out to be too difficult or time-consuming to do.

Clinical psychological investigations can be divided into three types. The first type, usually the simplest, merely attempts to describe the patient in terms of some well-known and more or less clearly defined variable, for example in the assessment of intelligence. Even here it is easy to slip into a familiar routine and prescribe for instance the Wechsler for every case, since it is a fairly well standardized and reliable test. Again, however, this is not an experimental approach. Our investigations, to be experimental, must try to answer some particular question. We must find out *why* the doctor wants an estimate of the patient's intelligence before we can start a sensible enquiry. Sometimes the doctor only wants to discover whether the patient can understand therapeutic interviews. Sometimes he has a more specific aim. He may, for example, wish to advise the patient whether he should return to university. Often we find that the test is used to predict some future behaviour. We must find out what particular intellectual factors are relevant and measure these. If we are to advise whether the patient should return to university we must give tests of those aptitudes which are known to be relevant to the particular course the patient will take. Our purpose is not to compare his intelligence with people in general (as we would do from the Wechsler) but to compare his intelligence and relevant aptitudes with those of a very specific university population. When we have made our measurement, however, we must also take into account the *errors* of measurement and errors of prediction of the tests we are using. We must be able to assess these in making our predictions and so we must make use of tests for which there are adequate data. There can be no measurement or prediction in science without error, and the clinical psychologist must have the statistical means for taking this into account.

Sometimes of course, we wish to describe the patient's intellectual capacities, not to predict future behaviour, but to find out whether some decline has occurred. If we lack pre-illness measures, at least we can use tests for which there are data concerning individuals of this particular education and occupational training, so as to determine how he now compares with a population of which he was formerly clearly a member.

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A second group of investigations involves determining the *abnormality* of a certain function. For example, the psychiatrist may wish to know whether an elderly patient's memory is bad enough to be regarded as a symptom. Such questions in general involve first of all measuring the function, and then comparing the individual's score with those obtained in normal people of the same age, sex and intelligence. If the score falls outside this distribution it is clearly "abnormal". Sometimes we have standardized tests available with norms which enable us to make this judgment. Often, however, we do not. We do not, for example, have a test of long-term retention which has been standardized on old people, although tests are available which provide some data for young people. In order to answer the question, we must ourselves test a sample of normal old people of the same intelligence and age and compare our patient's score with these. If no standard test exists at all, we might make one up and collect the necessary control data for it. This type of problem is not difficult although it may be time-consuming.

In any such investigation we are again faced with the problem of errors of measurement. If we use tests for assessing postulated abnormalities for which there are no adequate data, or if we make up tests ourselves, we cannot be sure about the *reliability* of our findings. Again we can only fall back on the experimental approach. If the abnormality really exists it should manifest itself in numerous situations. If we predict that the patient will fail in a series of rather different types of tasks and not in others, and all our predictions are confirmed, the reliability of our measures is confirmed and the hypothesis supported. Purely descriptive investigations of this sort again can be very time-consuming. An example is an investigation carried out on a girl of 21 who was a known post-encephalitic. She had a very severe memory disorder, the nature of which was investigated experimentally. It was shown during a long course of experiments that her failure to recall material was due to her inability to *learn* it in the first place. An interesting discrepancy was observed. Her ability to learn meaningful material (i.e. words or pictures) by the paired associate method, was grossly impaired, while her ability to learn meaningless material (i.e. nonsense syllables or nonsense drawings) was normal. To arrive at the conclusions about the abnormality of her learning, control data were used for some standardized research tests and a control subject was tested for part of the experiments. To arrive at the generalization about the discrepancy between meaningful and meaningless material, however, a long series of learning experiments were conducted. It had to be established that the observed differences in learning rate were not due to such factors as the differences between recall and recognition, the sensory modality involved, the type of material, and so on. The results showed a consistent difference between meaningless and meaningful material, ruling out the possibility that the initial result was due merely to error of measurement.

The third type of clinical investigation is concerned with the *cause* of an established abnormality. These investigations are the most difficult and not all that we have done have been successful. In setting up explanatory hypotheses in a clinical investigation the same rules should apply as apply in ordinary scientific research. In many cases it is possible to think of several hypotheses which can account for the known facts and thus seem equally likely. It is difficult to know which to test first. Other things being equal, the best hypothesis is that which yields the greatest number of predictions. An hypothesis from which *nothing* follows except the facts it seeks to explain, is valueless. This seems to be the commonest fault in much psychiatric theorizing.

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Let us consider a concrete example of these two sorts of theories. A boy who came to the Children's Department had two main problems (1). First of all he was emotionally and behaviourally disturbed, particularly at school. Secondly, although 10 years of age, he was completely unable to read despite considerable special tuition of the usual kind. As his symptoms were improving, the investigation concentrated primarily on his reading difficulty which still remained. Preliminary testing demonstrated that his general intelligence was low average, so that this could not account for his disability. He was not more backward in other subjects than one would expect, so that the disability appeared relatively specific. He did not have any sensory defects (deafness or poor eyesight) which could account for it. In the end it would have been possible to formulate two hypotheses: (1) This was a case of congenital word blindness, and (2) This was a boy who was abnormally bad at associating one perception with another: e.g. printed patterns and spoken words.

It was decided to *ignore* the first (in fact it was not seriously considered by the investigators), and test the second hypothesis. The hypothesis of congenital word blindness was quite tenable. It accounted for all the facts—the specificity of the backwardness, the lack of peripheral sensory defects, and the complete inability to learn. However, it had few implications. Very little else could be *predicted* from it. Since congenital word blindness is said to be due to damage in a specific cortical area, it might have been predicted that tests of *brain* damage would yield positive results. However, nothing *else* would follow from the theory even if this prediction were verified. True this formulation has a pessimistic tone, suggesting that treatment would be difficult or impossible, but even this is not a clear prediction, as other congenital defects (e.g. deformed feet) can be treated and cured. The word congenital does not mean unalterable. In fact *nothing* follows from this hypothesis. It is relatively sterile.

The other hypothesis has a number of implications, however. If true, it should affect learning in general. A series of experiments were started to determine whether or not the patient *did* have abnormal difficulty in forming connections between sensory modalities. It was found that indeed he did. By comparison with a control group of readers, it was shown that he was abnormally bad at forming connections within the auditory modality, within the kinaesthetic modality, and between these modalities. He was *normal* at forming connections within the visual modality. It was also found that he was *ultimately* able to learn connections in *any* modality if given enough repetition. That is, he had no defects of retention. It was concluded that these learning defects could account for his failure to learn to read by normal methods. The special coaching programme which was developed for him emphasized the formation of visual associations and repetition. He ultimately learned to read. The first hypothesis, that of congenital word blindness, was never tested. It may well be correct. However, as an hypothesis, whether correct or not, it is virtually useless as so little follows from it. It is not really necessary.

Most of the examples I have quoted were especially time-consuming as they involved the testing of a control group also. This is clearly needed if we are to discover whether or not a function is to be regarded as *abnormal*. However, a control group is not always needed in an explanatory investigation. If we accept the abnormality of a symptom, the subject can be used as his *own* control. That is we seek those experimental situations which improve the patient's *own* performance. Thus he acts as his own control and provided we take care to rule out practice effects, this can be adequate. We still do not know of course

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whether a control group would do the same under similar circumstances, but if our treatment is successful the question is irrelevant for that case.

SUMMARY

A general experimental approach to clinical psychological practice has been outlined and it has been suggested that experimental method in clinical psychology is both possible and useful in solving some of the problems in a very difficult field.

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